Anti-Harassment Policy and the Startup Labor Market

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Abstract

This paper examines how anti-harassment legal reforms that weaken non-disclosure agreements (NDAs) in cases of workplace sexual harassment affect startups' hiring and organizational decisions. Using a staggered difference-in-differences design and LinkedIn data on over 50,000 U.S. venture-capital-backed startups from 2014–2022, we find that NDA reforms, although intended for employee protection, reduce female hiring by about 8%, with effects concentrated among junior women, who are statistically more prone to sexual harassment, and in small or male-dominated startups. The results apply to both the intensive and extensive margins of female hiring. Treated entrepreneurial firms also witness more departures of male managers, promote more women, and receive less VC funding. These results suggest that while NDA-weakening laws increase firms' perceived legal risk and reduce female hiring, they also trigger internal restructuring that promotes women's advancement into leadership and may, over time, foster more accountable and inclusive organizational cultures.

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1 Introduction

Gender inequality in labor markets remains one of the most persistent challenges in modern economies. Beyond well-documented differences in human capital and occupational sorting, recent work highlights a fundamental but less visible source of inequality—workplace sexual harassment and the institutional arrangements that shape how it is handled (Folke and Rickne, 2022). Harassment not only imposes direct psychological and economic costs on victims (Welsh, 1999; McDonald, 2012; Fitzgerald and Cortina, 2018), but also deters women from entering, remaining, and advancing in certain industries and firms. In particular, startups face acute gender inequality (Ewens and Townsend, 2020; Calder-Wang and Gompers, 2021) and elevated rates of workplace harassment.[1]

One institutional mechanism that has allowed harassment to go unreported and unaddressed is the widespread use of non-disclosure agreements (NDAs) in employment contracts.[2] By silencing victims and concealing information, NDAs help shield firms from reputational damage and simultaneously allow hostile workplace cultures to persist. In the aftermath of the #MeToo movement, beginning in 2018, a wave of U.S. states—including New York, California, and Washington—passed laws weakening or nullifying NDAs (NDA-weakening laws hereafter) that restrict disclosure of sexual harassment. These state-level initiatives culminated in the federal Speak Out Act of 2022, which made pre-dispute NDAs unenforceable nationwide for claims of sexual assault and harassment.

In this paper, we leverage the staggered passage of state-level NDA-weakening laws as an experiment to study their effects on the hiring, promotion, and retention of female workers in venture-capital-backed (VC-backed) startups. This setting is particularly informative: startups are disproportionately important to innovation and job creation, yet most lack

[1] For example, the Women in Tech 2023 survey reports that 40% of female founders have experienced sexual harassment, 65% report being told they would be more likely to get funded if they were male or had a male co-founder, and 48% of female tech workers report having experienced harassment. See womenwhotech.org/data-and-resources/state-women-tech-and-startups-2023.

[2] Although NDAs were originally designed to protect trade secrets and proprietary information, firms have frequently used them to prevent employees from speaking publicly about internal misconduct, including cases of sexual harassment and assault. The next section discusses in greater detail the background of NDAs and sexual harassment.

the formal human resources infrastructure—employee training, compliance staff, dedicated reporting channels—that can prevent harassment or contain it before it escalates to public disclosure or litigation. Their success also depends heavily on reputation and investor confidence, making them acutely sensitive to the reputational and legal exposure that NDA-weakening laws create. By empowering victims to speak publicly, these reforms could deter misconduct and make startups more attractive to female workers.

At the same time, the reforms raise the expected legal and reputational cost of harassment for all firms that employ women—not only those where misconduct has occurred—and could reduce firms' demand for female workers, especially in startups with limited internal compliance capacity. This tension between protective intent and adverse employment effects echoes a pattern documented for other labor protections (Acemoglu and Angrist, 2001; Doleac and Hansen, 2020), though the mechanism here operates through transparency rather than information frictions or firing costs.

We assemble a large-scale dataset linking over 50,000 VC-backed U.S. startups from PitchBook with worker-level employment data from LinkedIn (via Revelio Labs) for the period 2014–2022. Combining these two data sources allows us to observe detailed hiring flows, job titles, seniority, and educational background for employees within each startup over time. We exploit the staggered passage of state-level NDA-weakening laws between 2018 and 2022 in a difference-in-differences (DiD) framework, comparing startups headquartered in treated states to those in untreated states before and after the reforms. Before the enactment of NDA-weakening laws, the vast majority of harassment went unreported: 85% of victims never filed a formal legal charge, and approximately 70% did not report the incident internally (Feldblum and Lipnic, 2016). Following their passage, Holstead, Huang, and Pinto (2025) document a significant increase in employee-sourced media coverage of firms, with a more negative tone—evidence that the reforms encouraged disclosure of workplace misconduct.

We first document a sharp decline in female hiring following the enactment of NDA-weakening laws. Startups headquartered in treated states (treated startups hereafter) hire 8% fewer women per year than startups in states that do not enact NDA-weakening laws (control startups hereafter). The effect is statistically significant at the 1 percent level and represents an economically meaningful reduction in female hiring for young firms where early workforce composition shapes organizational culture and growth. The decline spans both high- and low-skilled women, with no significant difference between the two groups. There are no divergent trends in female hiring between treated and control startups in the four years prior to NDA reforms; the negative effect appears sharply in the first year after enactment and persists without attenuation during our sample period.

The negative effect applies to both the intensive and extensive margins of hiring. Conditioning on hiring any female workers, the number of women hired per startup declines by 3%. On the extensive margin, the likelihood that a startup starts hiring any female worker at all falls by nearly five percentage points in the four years following NDA reforms. The laws thus reduce both the depth and breadth of female participation in the startup workforce.

We present several results consistent with a demand-side mechanism: facing higher expected costs of harassment, startups reduce female hiring where their legal and reputational exposure is greatest. The decline concentrates sharply among junior female workers, consistent with prior evidence that junior women face disproportionate harassment exposure (Cortina and Berdahl, 2008), while hiring of senior women remains largely unchanged. Cross-sectional tests reinforce this interpretation: small startups and those with low pre-existing female representation—firms with the weakest internal capacity to manage harassment risk—exhibit much larger declines in female hiring, while women-friendly or larger startups show weaker or no response.

The baseline estimates are robust to a stacked regression estimator (Cengiz, Dube, Lindner, and Zipperer, 2019; Baker, Larcker, and Wang, 2022) that addresses concerns about heterogeneous treatment effects in staggered designs (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), and to excluding the Covid-19 period and influential states such as California and New York. Most importantly, a placebo analysis using startups' hiring outside their treated headquarters state reveals no change in female employment—ruling out firm-level confounds and confirming that the effect is driven by state-level legal exposure.

An alternative explanation is that heightened awareness of harassment in reforming states—rather than the laws themselves—reduced women's willingness to work in startups. While we cannot fully rule out supply-side effects, several tests suggest they are unlikely to be the primary driver. Directly controlling for state-level awareness of harassment—measured by the Google search index for "Metoo"—has no predictive power for female hiring, and the baseline estimates remain nearly identical. A supply-side channel also predicts that the gender salary gap should narrow as women become harder to recruit; we find no evidence of such narrowing. Most directly, the 2022 federal Speak Out Act, which imposed NDA-weakening legislation on previously untreated states through a federal mandate unrelated to any individual state's level of awareness, produced a comparable decline in female hiring of approximately 14–15%. On balance, the evidence points to the legal framework itself, and the resulting increase in firms' expected costs, as the primary channel.

The silver lining is that we observe meaningful organizational restructuring within treated startups. Firms promote more women to managerial positions after NDA-weakening laws take effect, and more male managers depart. Notably, departing male managers transition to jobs with lower pay and lower seniority, and experience longer unemployment spells—suggesting that many of these separations are involuntary rather than voluntary moves to better opportunities. These patterns are consistent with startups reconfiguring their internal hierarchies in response to higher legal risks: removing potential harassers from management and elevating women into leadership roles. Together, the results suggest that while NDA-weakening laws reduce female hiring in the short run, they also catalyze internal changes that could, over time, foster more inclusive and accountable workplaces. Treated startups also experience a decline in the likelihood of raising new venture capital rounds, suggesting that the legal reforms impose broader frictions on startup operations.

Although the evidence is more supportive of the demand-based mechanism, it does not rule out the possibility that NDA-weakening laws enhance women's willingness to work and, hence, the supply of female workers. The evidence simply demonstrates that the laws' negative demand effects outweigh their positive supply effects in our sample period. As the organizational changes we document gradually take effect, the laws' positive supply effects may outweigh their negative demand effects in the long run.

A growing body of literature highlights the workplace environment and culture as important determinants of gender gaps in labor markets. While differences in human capital and occupational choice remain central (Goldin, 2014; Blau and Kahn, 2017), recent work points to sexual harassment and power asymmetries as critical barriers to women's labor market participation (Cortina and Berdahl, 2008; McLaughlin, Uggen, and Blackstone, 2017). Harassment deters entry, drives attrition, and perpetuates under-representation, particularly in male-dominated and high-growth institutions, where persistent barriers also prevent women from advancing to senior leadership (Azmat, Cuñat, and Henry, 2025). Scholars have also shown that organizational silencing mechanisms—such as confidentiality clauses, mandatory arbitration, and limited whistle-blower protection— discourage reporting and allow misconduct to persist (Hersch, 2015; Folke and Rickne, 2022). We contribute to this literature by studying how legal reforms that weaken NDAs—a key institutional form of organizational silencing—affect firms' actual employment and promotion of women at scale. In the talent-allocation framework of Hsieh, Hurst, Jones, and Klenow (2019), the aggregate gains from anti-discrimination progress come from women sorting into high-productivity occupations; our findings imply that NDA-weakening laws, by deterring female hiring in the disproportionately innovative VC-backed startup sector, work against this allocative channel and thus may carry potential aggregate-productivity costs that extend beyond the firm-level effects we document.

Our work also adds to the literature on the unintended consequences of labor-market regulations. The Americans with Disabilities Act reduced employment of disabled workers (Acemoglu and Angrist, 2001), "Ban-the-Box" laws reduced hiring of workers with criminal records (Doleac and Hansen, 2020), and the #MeToo movement reduced male–female research collaborations (Gertsberg, 2026)—in each case, well-intentioned protections decreased employment of the workers they aimed to help, because firms internalized higher expected costs of legal compliance. Related theoretical frameworks (Lazear, 1990; Autor, III, and

Schwab, 2006) emphasize that hiring declines when employment protection increases downside risk without improving monitoring. NDA-weakening laws operate through a distinct channel: by enhancing the transparency of harassment incidents, they heighten firms' exposure to reputational costs and legal risks (Hersch, 2015). Our study extends this literature to the domain of gender and harassment protection.

A related line of research examines how firms respond to labor-related regulations and how internal governance mediates those responses. Bena, Ortiz-Molina, and Simintzi (2022) find that firms invest in cost-saving technologies when state-level legal changes raise labor dismissal costs, and Bennedsen, Simintzi, Tsoutsoura, and Wolfenzon (2022) show that a Danish wage-transparency law shrinks the gender pay gap by slowing male wage growth. More broadly, prior studies show that stronger governance and gender diversity improve monitoring and firm performance (Adams and Ferreira, 2009; Gompers, Mukharlyamov, and Xuan, 2016; Levi, Li, and Zhang, 2014; Tate and Yang, 2015; Huang and Kisgen, 2013), while weak internal oversight fosters ethical lapses and misconduct (Dyck, Morse, and Zingales, 2010). We show that the effects of NDA-weakening reforms are concentrated in small, male-dominated startups—likely firms with weaker internal governance—complementing evidence that institutional strength mediates the effectiveness of transparency and accountability policies (Hutton, Jiang, and Kumar, 2015; Kedia and Rajgopal, 2011).

Finally, this paper contributes to the literature on entrepreneurial finance and diversity. Research on venture capital and startup ecosystems documents persistent gender disparities in founding and hiring (Calder-Wang and Gompers, 2021; Guzman and Stern, 2020), as well as gender-based differences in the extent to which fundamental frictions in venture capital markets can be mitigated (Howell and Nanda, 2024). We add a legal dimension, showing that labor-market regulation interacts with financing frictions: startups exposed to NDA-weakening laws not only hire fewer women but also raise less venture funding afterward. The evidence links workplace regulation to capital-market outcomes, bridging insights from labor economics, entrepreneurial finance, and law and economics.

2 Sexual Harassment and NDA Reforms

2.1 Sexual Harassment In the Workplace

Sexual harassment and assault are pervasive in the U.S. workplace. A 2019 national survey reports that 81% of women and 43% of men have experienced some form of sexual harassment or assault during their lifetime, and 38% of women and 14% of men have experienced it at work (Kearl, Johns, and Raj, 2019). The technology sector exhibits a similar pattern: 42% of women and 24% of men employed in high-tech firms report workplace harassment.[3]

Although workplace sexual harassment and assault are forms of sex discrimination prohibited by Title VII of the Civil Rights Act of 1964, the vast majority of victims do not report the conduct, and fewer still file formal complaints within their institutions (Cortina and Berdahl, 2008). Fear of blame, disbelief, and retaliation deters reporting; roughly two-thirds of victims who do come forward face retaliation of some kind, and their disclosures are typically met with organizational indifference or inaction (Cortina and Berdahl, 2008).[4]

Women bear a disproportionate share of this burden. They filed 78.2% of the 27,291 harassment charges submitted to the EEOC over 2018–2021, and their harassers are most often colleagues, supervisors, and senior managers within the same firm (Feldblum and Lipnic, 2016).

2.2 NDAs, NDA Reforms, and Sexual Harassment

Non-disclosure agreements (NDAs) originated in the 1940s as contractual tools to protect firms' trade secrets and other proprietary information.[5] Their use expanded during the post-war growth of technology industries—IBM, for example, relied on NDAs to safeguard

[3] https://womenwhotech.org/data-and-resources/state-women-tech-and-startups-2023.

[4] Sexual-harassment and retaliation charges are tightly linked in administrative data: 43.5% of the 27,291 sexual-harassment charges filed with the U.S. Equal Employment Opportunity Commission (EEOC) between 2018 and 2021 were accompanied by a retaliation charge. See https://www.eeoc.gov/data/sexual-harassment-our-nations-workplaces.

[5] https://www.cjr.org/special_report/nda-agreement.php.

intellectual property against competitors—and today most employers require new hires to sign some form of NDA as a condition of employment. In this original domain, NDAs serve a legitimate governance function. Their extension to harassment and discrimination claims, however, creates a trade-off between protecting firm reputation and preserving victims' ability to speak.

Employment NDAs now commonly take two forms.[6] *Pre-dispute* NDAs, imposed as a condition of employment, require workers to waive the right to publicly discuss discrimination, harassment, or other workplace misconduct. *Post-dispute* NDAs, signed as part of settlement agreements, bind victims to confidentiality in exchange for compensation, concealing misconduct, impeding collective action among workers, and shielding perpetrators from reputational accountability. The case of film producer Harvey Weinstein is emblematic: confidential settlements silenced accusers for decades and allowed the misconduct to continue, until investigative reporting in 2017 brought it to light.[7]

Because employment contracts are typically confidential, direct estimates of NDA prevalence are scarce. Available evidence suggests that NDAs are especially widespread among high-technology startups, whose core assets are largely intangible.[8] Venture capital investors actively promote their use: the National Venture Capital Association (NVCA), the industry's principal trade association, includes an NDA clause in the model term sheet that anchors contracting norms across U.S. venture capital:[9]

> [Non-Disclosure and Developments Agreement:] Each current and former Founder, employee and consultant will enter into a non-disclosure and proprietary rights assignment agreement in a form reasonably acceptable to the Investors.

[Insert Table 1 Here.]

The #MeToo movement, beginning in 2017, drew sustained public attention to the role

[6] See https://nwlc.org/wp-content/uploads/2020/04/NDA-Factsheet-4.27.pdf and https://hbr.org/2018/01/ndas-are-out-of-control-heres-what-needs-to-change.

[7] https://www.newyorker.com/news/news-desk/harvey-weinsteins-secret-settlements.

[8] https://www.lexagle.com/blog-en-sg/non-disclosure-agreements-ndas-in-the-startup-ecosystem-pitfalls-and-best-practices.

[9] https://nvca.org/document/nvca-2020-term-sheet-4/.

of NDAs in enabling serial workplace misconduct. Between 2018 and 2022, fourteen states enacted legislation restricting or voiding NDAs that suppress claims of sexual harassment, assault, or discrimination. The statutes vary in scope—some prohibit NDAs in settlement agreements involving harassment (e.g., California's SB 820 and New York), while others bar employers from requiring NDAs as a condition of employment (e.g., Washington)—but all weaken the enforceability of NDAs over harassment-related disclosures. Table 1 lists the fourteen states and the effective dates of their reforms. The federal Speak Out Act, enacted in December 2022, subsequently extended a minimum national standard by voiding pre-dispute NDAs covering sexual harassment and assault.

These reforms bind VC-backed startups headquartered in reforming states. California Labor Code § 925 and analogous provisions elsewhere prohibit employers from requiring employees who work primarily within the state to accept out-of-state choice-of-law clauses in their employment contracts. Firms therefore cannot evade reforming-state statutes by drafting NDAs under Delaware or other non-reforming law. In our setting, treatment status is determined by the location of the startup's workforce, not by its state of incorporation.

The enactment of these reforms is not random. States more receptive to NDA reform tend to exhibit stronger #MeToo mobilization, a more worker-protective policy orientation, and more active women's-rights advocacy; such states are also more likely to adopt other policies that improve female labor market outcomes. This correlation creates an omitted-variables concern, but one that works against the effect we document: unobserved women-protective policies in reforming states would, if anything, raise female hiring among in-state startups and attenuate any negative effect of NDA reform. Parallel pre-trends in female hiring between VC-backed startups in reforming and non-reforming states (shown in Section 4) further mitigate the concern that concurrent state-level factors, rather than the reforms themselves, drive our findings.

Taken together, these state reforms represent a substantial shift in the institutional protection of harassment disclosures. They lower the cost to victims of speaking publicly and have led to a measurable increase in news coverage of employer legal issues arising from employee disclosures (Holstead et al., 2025).[10] For firms, they raise the expected reputational and legal costs of harassment incidents, sharpening incentives to manage workplace conduct ex ante rather than contain it ex post.

3 Sample Construction and Summary Statistics

3.1 Data

We construct our sample of VC-backed startups over 2014–2022 from the PitchBook database, a comprehensive venture capital database that is widely used in prior work on VC-backed startups. Its coverage has been strong since 2002, owing in part to the broad availability of Form D filings on the Securities and Exchange Commission (SEC) website (e.g., Babina et al., 2025).

Our main analysis focuses on 2014–2022, a nine-year window centered on the adoption of state-level laws that began weakening non-disclosure agreements in 2018. Starting the sample in 2014 gives us at least four years of pre-period data before the first wave of state reforms (Table 1). We end the sample in 2022, when the federal government enacted legislation that weakened NDAs nationwide.[11]

We measure startup employment and hiring using individual-level résumé data from Revelio Labs, which aggregates more than one billion online profiles, primarily from LinkedIn, and provides broad coverage of the U.S. workforce. The data are especially representative of professional, managerial, and technical occupations (Hampole, Papanikolaou, Schmidt, and Seegmiller, 2025), which closely match the workforce composition of VC-backed startups. Revelio covers roughly 90% of white-collar workers and 70% of the overall U.S. labor force (Hampole et al., 2025; Revelio Labs, 2022).

[10] Administrative evidence corroborates the rise in disclosure. Sexual-harassment charges filed with the EEOC rose 13.6% from FY2017 to FY2018, reversing a multi-year decline, and EEOC-initiated harassment lawsuits rose roughly 50% in the same year.

[11] Congress passed the Speak Out Act on November 16, 2022, and President Biden signed it into law on December 7, 2022. The Act limits the enforceability of pre-dispute non-disclosure and non-disparagement clauses related to sexual assault and sexual harassment claims.

Revelio infers gender using a probabilistic model based on first names trained on U.S. Social Security Administration data. For each name, the algorithm estimates the probability that the individual is female and classifies profiles with a predicted probability above 50% as female. For the vast majority of profiles, the predicted probability is close to either zero or one (Revelio Labs, 2022; Dorn et al., 2025).

Recent studies have used and validated Revelio data. For example, Ahn, Hoitash, Hoitash, and Krause (2025) study career trajectories and racial and gender promotion gaps in Big Four accounting firms. Lin, Shen, Shi, and Zeng (2025) examine how state adoption of Targeted Regulation of Abortion Providers (TRAP) laws affects women's job mobility. Gao, Ma, and Xu (2025) compare their main results based on Revelio with corresponding estimates from the U.S. Census LEHD data and show that the Revelio-based results are robust. We use Revelio rather than the U.S. Census LEHD data for two reasons. First, Revelio provides richer worker-level characteristics that are central to our analysis, including gender, education, skills, and job titles. Second, we use firm webpages in Revelio to merge workers to VC-backed startups in PitchBook.

3.2 Sample Construction

We begin with the universe of 85,588 U.S. startups in PitchBook that received at least one round of VC financing and have non-missing founding-year information. We then link these firms to Revelio using two identifiers: company website URLs (e.g., "www.xyz.com") and LinkedIn company-page URLs (e.g., "www.linkedin.com/company/xyz/"). Both identifiers are available in PitchBook and Revelio, although each contains missing values. Among U.S. VC-backed startups founded between 2009 and 2022 in PitchBook, 97.4% report a company website and 73.7% report a LinkedIn page, so only 2.2% lack both identifiers. Because company websites are more widely available than LinkedIn URLs, we prioritize website-based matching.[12] Using this approach, we match 66,107 startups through company websites and an additional 5,406 through LinkedIn pages.

[12] The results are nearly identical when we instead prioritize LinkedIn URLs in the matching procedure.

We then construct a startup-year panel for 2014–2022. A startup enters the sample when it is between age zero (its founding year) and age five. It exits once it is older than five years or when PitchBook reports that it has gone bankrupt, been acquired, or gone public through an IPO. The final sample contains 52,607 unique VC-backed startups and 207,469 startup-year observations.

3.3 Sample Description and Summary Statistics

Table 2 reports summary statistics for startup characteristics and hiring during the first five years after founding. The average startup is 2.3 years old, completes about one VC round per year, and raises $6.8 million annually. About 59% of the 207,469 startup-years are in the fourteen states that enacted NDA-weakening laws between 2018 and 2022. This share is plausible because California, New York, and Washington—three states with large VC startup sectors—are among the adopters.

Our main treatment variable, $NDAWeaken_{it}$, equals one if startup i's headquarters state has enacted an NDA-weakening law by year t, and zero otherwise. The variable has a mean of 0.3, implying that 30% of startup-year observations fall in post-enactment years for firms headquartered in the fourteen treated states.

[Insert Table 2 Here.]

Startup employment is highly skewed: the average startup has 12.8 employees, whereas the median has only 4. The average startup hires 2.93 workers per year in its headquarters state, of whom 1.12 (34%) are women. These figures indicate that young startups typically hire only a small number of workers each year as they scale. We focus on hiring in the headquarters state for two reasons. First, aggregating hires across states would blur the identifying variation generated by state-level NDA laws. Second, young startups rarely expand hiring outside their headquarters state during the first few years after founding.

We also examine the types of women startups hire, with particular emphasis on skill and seniority. We classify female hires as high- or low-skilled using two alternative measures: education and occupation. Based on education, we define high-skilled women as those with at least a bachelor's degree and low-skilled women as those without one. Based on occupation, we use O*NET Job Zones and classify workers in occupations requiring medium, considerable, or extensive preparation (Job Zones 3–5) as high-skilled, and those in occupations requiring little or some preparation (Job Zones 1–2) as low-skilled (see, e.g., Belo, Li, Lin, and Zhao, 2017; Dey, Haslag, Sensoy, and White, 2025).

On average, a startup hires 0.84 high-skilled women and 0.12 low-skilled women per year when skill is measured using education, and 0.88 high-skilled women and 0.12 low-skilled women when skill is measured using occupation.[13] These patterns suggest that VC-backed startups predominantly hire high-skilled women, a group likely to be particularly important for early-stage growth.

We further divide newly hired women into junior and senior workers using employee age and job seniority. Following prior work (e.g., Kogan, Makarov, Niessner, and Schoar, 2024; Agarwal et al., 2025), we classify workers younger than 35 as junior and those older than 35 as senior. Because LinkedIn users do not report age directly, we infer age from education histories in Revelio and can do so for roughly 50% of workers in our sample. We also classify seniority using job titles reported in Revelio: entry- and junior-level positions are coded as junior, whereas associate, manager, director, executive, and senior executive positions are coded as senior. On average, a startup hires 0.47 junior women and 0.09 senior women per year when seniority is measured using age, and 0.66 junior women and 0.47 senior women when seniority is measured using job titles.

4 Female Hiring After NDA-Weakening Laws

This section investigates how NDA-weakening state laws affect VC-backed startups' hiring of female workers. We first estimate the baseline effect on female hiring using a staggered difference-in-differences design and assess robustness across specifications, skill levels, and alternative estimators. We then validate identification through a placebo test on hires in

[13] The sum of high- and low-skilled female hires is smaller than the average annual number of female hires because education and occupation data are missing for some workers.

non-headquarters states and decompose the effect along the extensive margin. Next, we examine the mechanisms behind the baseline results, focusing on worker seniority, the strength of firms' internal employee protections, and a supply-side alternative. Finally, we assess the consequences of these laws for startups' venture capital funding.

4.1 Baseline Results

To identify the effects of NDA-weakening laws on VC-backed startups' hiring of female workers, we exploit the staggered enactment of these laws across fourteen states between 2018 and 2022 and estimate the following difference-in-differences model:

$$Y_{it} = \beta NDAWeaken_{it} + \theta Log\ Firm\ Age_{it} + \alpha_i + \tau_t + \epsilon_{it}. \tag{1}$$

The dependent variable Y_{it} is the number of female workers hired by startup i in year t. $NDAWeaken_{it}$ is an indicator equal to one if the headquarters state of startup i has enacted an NDA-weakening law by year t, and zero otherwise; its coefficient β is the parameter of interest. We include the log of firm age as a control, along with startup (α_i) and year (τ_t) fixed effects. Standard errors are clustered at the headquarters-state level.

[Insert Table 3 Here.]

Table 3 reports the estimates from model (1) over the 2014–2022 period. We use a Poisson model as our main specification because the dependent variable is a count with many zeros (Cohn, Liu, and Wardlaw, 2022), and Poisson pseudo–maximum likelihood (PPML) estimators remain consistent under heteroskedasticity with high-dimensional fixed effects (Silva and Tenreyro, 2006). Column (6) reports OLS estimates using the fraction of female hires as the dependent variable.

The results in Table 3 show that startups hire significantly fewer women after the headquarters state adopts NDA-weakening laws. The coefficient on $NDAWeaken$ is -0.081 in columns (1) and (2), with and without controlling for startup age. Startups in treated states thus hire 7.8% ($= e^{-0.081} - 1$) fewer women per year relative to control startups.

This magnitude is economically meaningful for young startups, where early workforce composition shapes organizational culture and growth. Given that women already constitute only 34% of startup hires, the laws further widen the gender gap in entrepreneurial firms. The baseline results remain qualitatively unchanged when we exclude from the sample the years before the startup starts to hire any employees (column (3)), and when we extend the sample period to include the first seven years after the startup's founding year rather than five years (see columns (1) and (2) of Table IA.1).

Two features of our sample period warrant additional checks. First, the COVID-19 pandemic beginning in 2020 overlaps with the post-treatment window for most treated states. Although year fixed effects absorb common macroeconomic shocks, the pandemic could confound our estimates if it differentially affected hiring in states that enacted NDA-weakening laws. Column (4) of Table 3 shows that the results are robust when we exclude the post-2019 period. The economic magnitude is somewhat smaller, reflecting the fact that the sample includes only one post-treatment year. To further address concerns about potential COVID confounding, we also exploit the 2022 federal change and focus on a post-COVID sample (2020–2024), obtaining qualitatively similar results (see Section 4.4.3 below). Second, California and New York—both early adopters (2018) and home to a disproportionate share of VC-backed startups—could exert outsized influence on the estimates. Column (5) excludes startups headquartered in these two states; the coefficient remains robust, indicating that the results are not driven by a few large startup hubs.

A natural concern is that the decline in female hiring reflects a broader contraction in startup employment rather than a gender-specific response to NDA-weakening laws. We provide several tests to strengthen the interpretation of decreases in female hiring. First, we replace the dependent variable with the fraction of women among all workers hired by the startup in the year (column (6) of Table 3). The coefficient on $NDAWeaken$ is -0.012 and statistically significant at the 1% level, implying that the female share of hires drops by 1.2 percentage points—a 3.5% decline relative to the sample average of 34%.[14]

In the Internet Appendix, we estimate the effect on male hiring directly using a Poisson

[14] Our estimate is also robust to inference based on the wild cluster bootstrap.

regression analogous to columns (1) and (3) of Table 3. The coefficients, reported in columns (1) and (2) of Table IA.2, are statistically insignificant and economically small, indicating no meaningful change in male hiring. Moreover, we estimate the differential effect on female versus male hires by stacking gender-specific counts within each startup-year and interacting $NDAWeaken$ with an indicator $Female$. The interaction term, reported in columns (3) and (4) of Table IA.2, is negative and statistically significant at the 1% level, implying that treated startups hire 5–6% fewer women per year relative to men. Taken together, these results establish that NDA-weakening laws reduce female hiring specifically, not overall startup employment.

The "unintended consequences" literature finds that labor regulations often affect workers asymmetrically across skill levels. For example, Doleac and Hansen (2020) show that the employment effects of Ban-the-Box policies concentrate among low-skilled workers, while Autor et al. (2006) find that wrongful-discharge costs initially fall on less-educated workers but shift to more-educated workers over time. In our setting, the expected skill gradient is ambiguous. On one hand, VC-backed startups predominantly hire high-skilled women (0.84 per year vs. 0.12 low-skilled; Table 2), and these workers are critical for innovation and early-stage growth. They also tend to work in male-dominated technical roles where harassment exposure is elevated (Folke and Rickne, 2022). On the other hand, high-skilled women are harder to replace, which may make firms reluctant to reduce their hiring. We therefore decompose the baseline effect by skill level using two alternative classifications: educational attainment (bachelor's degree or higher vs. below) and O*NET Job Zones (Zones 3–5 vs. Zones 1–2).

[Insert Table 4 Here.]

Table 4 reports the results. The coefficient on $NDAWeaken$ is negative and statistically significant for both high-skilled (columns (1) and (2)) and low-skilled (columns (3) and (4)) female hires, under both classification schemes. The point estimates are somewhat larger for low-skilled women, but the differences are not statistically significant (columns (5) and (6)). The finding that NDA-weakening laws reduce female hiring across skill levels contrasts with settings such as Ban-the-Box, where information frictions concentrate effects on low-skilled workers. In the startup context, harassment-related legal exposure applies across skill levels, producing a broad-based reduction in the demand for women rather than adjustment along the skill margin alone. But our evidence is consistent with these earlier findings that low-skilled workers are more strongly impacted.

4.2 Alternative Specifications, Pre-trends, and Placebo Tests

We subject the baseline estimates to three tests that address distinct threats to the causal interpretation of our results.

A well-documented concern in staggered difference-in-differences designs is that already-treated units can serve as implicit controls for later-treated cohorts, biasing the two-way fixed effects estimator when treatment effects are heterogeneous (e.g., Baker et al., 2022; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Adhikari, Agrawal, and Sharma, 2026). In our setting, startups in early-adopting states such as California and New York (treated in 2018) enter the control group for cohorts treated in 2019, 2020, and 2022, potentially contaminating those estimates. To address this, we implement a stacked regression estimator: for each treatment cohort, we construct a separate event-time panel that includes only never-treated startups as controls, then stack the cohort-specific panels and estimate with cohort-specific fixed effects (Cengiz et al., 2019; Baker et al., 2022). The stacked regression yields a coefficient of approximately -0.090 (Table IA.3 in the Internet Appendix), close to the baseline estimate of -0.081 and statistically significant at the 1% level, confirming that our results are not an artifact of the staggered design.

The identifying assumption of our difference-in-differences design is that female hiring in treated and control states would have evolved similarly absent the NDA reforms. We assess this assumption by estimating dynamic treatment effects within the stacked regression framework, interacting a treatment indicator for the enactment of state NDA-weakening laws with event-time indicators defined relative to the laws' effective year. Figure 1 plots the estimates. In the four years preceding NDA reforms, the coefficients are economically small and statistically indistinguishable from zero, showing no evidence of diverging pre-trends. The estimates turn sharply negative in the first year after enactment and remain persistently negative over the subsequent three years, with no sign of attenuation. The sharp break at the reform date, combined with the absence of pre-existing trends, is consistent with a causal effect of the NDA-weakening laws on female hiring.

The event study rules out pre-existing time-series trends, but it cannot exclude the possibility that unobserved firm-level shocks—correlated with headquarters-state treatment timing—drive the results. We exploit the geographic structure of our setting to address this concern. Because NDA-weakening laws apply at the state level, they should affect a startup's hiring only in its headquarters state, not in other states where the startup also hires. If firm-level confounds (e.g., a founder departure or a product-market shock) were responsible for the decline in female hiring, we would expect to observe similar reductions in the startup's non-headquarters hiring as well.

[Insert Table 5 Here.]

We test this prediction by re-estimating model (1) with the dependent variable replaced by the number of female workers hired in the startup's non-headquarters states that did not adopt NDA-weakening laws. As reported in Table 5, the coefficient on $NDAWeaken$ is statistically insignificant and economically close to zero. The effect is thus localized to the legal jurisdiction where the NDA reform applies, ruling out firm-level confounds and reinforcing a causal interpretation of the baseline results.

4.3 Extensive Margin Analysis

The results so far indicate that VC-backed startups hire fewer female workers after the enactment of NDA-weakening laws. Because the number of female hires reflects both extensive and intensive margins, the baseline estimates may capture adjustments along either or both dimensions. Understanding whether NDA-weakening laws affect the extensive margin—the likelihood that a startup begins hiring women at all—is important for two reasons. First, a central finding in the unintended consequences literature is that protective labor regulations can deter firms from hiring protected workers entirely: the ADA reduced the employment of disabled workers (Acemoglu and Angrist, 2001), Ban-the-Box laws reduced hiring of workers with criminal records (Doleac and Hansen, 2020), and the #MeToo movement reduced male–female research collaborations (Gertsberg, 2026). Whether NDA-weakening laws similarly prevent startups from initiating female hiring is a direct test of whether these reforms produce comparable extensive-margin exclusion. Second, in the startup context, whether a firm begins hiring women is a consequential threshold: early workforce composition shapes organizational norms and referral networks, so a failure to hire the first female worker can have lasting effects on gender diversity within the firm.

To isolate the extensive margin, we restrict the sample to startups that had not yet hired any female worker and test whether NDA-weakening laws reduce the probability that they begin to do so. Because the relevant sample—startups with zero prior female hires—depends on when treatment occurs, the analysis requires a cohort-specific design. For each treatment year t in which at least one state enacted NDA-weakening laws, we assemble a cohort of VC-backed startups satisfying two criteria: (i) the startup is headquartered in a state that enacted NDA-weakening laws in year t (treated group) or in a state that had never enacted such laws (control group); and (ii) the startup had not hired any female worker prior to year t. We stack these cohort-specific samples, following the same logic as the stacked regressions described in Section 4.2, and estimate the following linear probability model:

$$Y_{i,[t,t+n]} = \beta\, NDAWeaken_{it} + \theta\, Frac\ Women_{s,t} + \gamma_g + \tau_t + \epsilon_{it}. \tag{2}$$

The dependent variable $Y_{i,[t,t+n]}$ equals one if startup i hires at least one female worker over the n years following treatment year t and zero otherwise. $Frac\ Women_{s,t}$ is the share of VC-backed startups in state s that had hired at least one female worker by year t; it absorbs cross-state variation in the baseline propensity to employ women. We also include firm age group fixed effects (γ_g) and treatment-year fixed effects (τ_t). We estimate the model for four values of n, ranging from one to four, and report the results in Table 6.

[Insert Table 6 Here.]

The coefficient on $NDAWeaken$ is negative and statistically significant in all four columns of Table 6. Startups in treated states are 2.8 percentage points less likely to hire their first female worker within one year of the law's enactment, and 4.9 percentage points less likely to do so within four years, relative to control startups. These estimates correspond to declines of approximately 10% within one year and 12% within four years, relative to the sample means (27.6% of startups that had not hired any female workers prior to the treatment years do so within one year, and 40.2% do so within four years). The monotonically increasing magnitude across the four windows indicates that the extensive-margin effect accumulates over time rather than reverting, consistent with the persistent treatment effects documented in Figure 1.

Together with the baseline estimates, these results indicate that NDA-weakening laws reduce both the breadth and depth of female participation in the startup workforce: treated startups hire fewer women (intensive margin) and are less likely to begin hiring women at all (extensive margin).

4.4 Why Do Startups Hire Fewer Women After NDA Weakening?

The baseline and extensive-margin results reveal that NDA-weakening laws reduce female hiring in VC-backed startups. We now examine the mechanisms behind this effect.

One mechanism operates through startups' demand for female workers. By allowing victims to disclose harassment publicly and pursue legal claims without being constrained by pre-dispute NDAs, these laws raise the expected cost of sexual harassment to the firm—through both litigation risk and reputational exposure. Crucially, the higher expected costs apply to all firms that employ women—not only those where harassment has occurred—because the laws expand every firm's exposure ex ante. Facing higher expected costs, startups may respond by reducing their hiring of women—the workers most likely to be victims of harassment and thus the source of incremental legal and reputational risk.

This demand-side mechanism generates two cross-sectional predictions. First, the reduction in female hiring should concentrate among junior women, who face disproportionate harassment exposure from senior male colleagues (Cortina and Berdahl, 2008) and thus represent the greatest incremental risk to the firm. This prediction is distinct from the skill-level decomposition in Section 4.1, which showed uniform effects across high- and low-skilled women: seniority maps directly to the harassment hierarchy, whereas skill level does not. Second, the effect should be stronger in firms with weaker internal protections against harassment. Firms with robust internal safeguards—such as compliance infrastructure and established reporting channels—can address harassment internally before it escalates to public disclosure or litigation, reducing their marginal exposure to NDA-weakening laws. We test these two predictions in Sections 4.4.1 and 4.4.2.

An alternative explanation operates through labor supply. States that enact NDA-weakening laws may do so because women there are already more aware of workplace sexual harassment, and this heightened awareness—rather than the laws themselves—could reduce women's willingness to work in male-dominated startups. Because both channels predict fewer female hires in treated states, the baseline estimates alone cannot distinguish them. The demand-side tests below are more naturally aligned with the demand channel, though the seniority pattern could also reflect supply-side self-selection. We test the supply-side alternative directly in Section 4.4.3, exploiting the 2022 federal Speak Out Act as a separate source of variation.

4.4.1 Effects by Worker Seniority

We test the first prediction by estimating model (1) separately for the startup's hiring of junior and senior female workers. As described in Section 3, we classify seniority using two measures: employee age (below vs. above 35) and job title (entry- and junior-level positions vs. associate through executive positions).

[Insert Table 7 Here.]

The results in Table 7 confirm the predicted pattern. The coefficient on $NDAWeaken$

is -0.095 (column (1)) and -0.102 (column (2)) for junior female hires, both statistically significant at the 1% level, implying that treated startups hire roughly 10% fewer junior women per year. For senior female hires, the coefficients are smaller in magnitude and statistically insignificant (columns (3) and (4)). Columns (5) and (6) formally test the difference by interacting $NDAWeaken$ with a junior indicator in a stacked regression; the interaction term is negative and statistically significant.

The concentration of the effect among junior women is consistent with the demand-side mechanism: firms reduce hiring where harassment-related legal exposure is greatest. That said, this pattern alone does not rule out the supply side, since junior women may also be more responsive to harassment concerns when choosing employers. The next test exploits variation in firms' internal protections, which provides a sharper test of the demand channel.

4.4.2 Strength of Internal Employee Protection

We test the second prediction by exploiting cross-sectional variation in the strength of startups' internal protections against sexual harassment. We construct two proxies. The first is firm size: smaller startups typically lack the resources to establish compliance infrastructure, such as employee training programs and dedicated human resources (HR) staff. The second is the pre-existing fraction of female employees, which serves as a revealed indicator of workplace quality—startups that have already attracted a high female share are likely to offer environments more conducive to women, whether through formal safeguards or informal culture. For each proxy, we split the sample into a top-third group (large or female-friendly) and a bottom-two-thirds group (small or male-dominated), and estimate model (1) separately for each subsample as well as on the full sample with an interaction term.

[Insert Table 8 Here.]

Table 8 reports the results. In columns (1) and (2), the coefficient on $NDAWeaken$ is -0.046 for large startups and -0.132 for small startups—nearly three times larger. Small startups thus hire 12.4% fewer women per year after the enactment of NDA-weakening laws, compared with 4.5% for large startups. The interaction term in column (3) confirms that the difference is statistically significant. Results are similar when we classify firm size using female employment rather than total employment (Table IA.4 in the Internet Appendix), and when we further disaggregate by size group: the effects concentrate in startups with fewer than ten employees and are insignificant for publicly listed tech firms (Tables IA.5 and IA.6 in the Internet Appendix).

Columns (4)–(6) repeat the analysis using the female employment share as the proxy. The results are even starker: the coefficient on $NDAWeaken$ is -0.007 for female-friendly startups—essentially zero—indicating that NDA-weakening laws have no detectable effect on startups that already employ many women. For startups with low female shares, the coefficient is -0.116, implying a 10.9% annual reduction in female hiring. The interaction in column (6) is positive and statistically significant.[15]

These cross-sectional patterns provide additional support to the demand channel previewed above. The supply-side alternative—that women self-select away from startups in treated states—cannot explain the large heterogeneity, because prospective employees generally cannot observe a startup's internal HR infrastructure or precise workforce composition before deciding to join. If the effect were supply-driven, we would expect roughly uniform declines across firms within a treated state.

Moreover, the female friendliness result runs in the opposite direction from the supply-side prediction. Prior to the NDA-weakening laws, male-dominated startups are the least attractive to women seeking legal protection, and should see the biggest supply-driven effect if the legal protections are established. Yet these are precisely the startups where female hiring falls the most. The concentration of the effect in small, male-dominated startups is instead consistent with the demand-side mechanism: firms with the weakest internal capacity to manage harassment risk respond to higher expected costs by reducing their hiring of women.

[15] We also investigate whether the gender of the startup's founder moderates the effect. Columns (4)–(6) of Table IA.4 in the Internet Appendix show that NDA-weakening laws reduce female hiring regardless of whether the startup has a female founder, suggesting that the relevant channel is institutional capacity rather than founder identity.

4.4.3 The Supply-Side Alternative

The cross-sectional tests above are more consistent with the demand channel than with supply-side self-selection, but they cannot definitively rule it out. We now provide a more direct test by exploiting the 2022 federal Speak Out Act as a separate quasi-experiment.

The core supply-side concern is one of reverse causality: states that enacted NDA-weakening laws may have done so because women in those states were already more aware of harassment, and it is this awareness—not the law itself—that reduced female hiring. Several features of the data argue against this interpretation. The event study in Section 4.2 shows no divergent pre-trends in the four years before enactment, and the treatment effect does not attenuate over time—contrary to what the supply-side mechanism would predict if awareness were converging across states. The 2022 federal Speak Out Act offers an even sharper test because it imposed NDA-weakening legislation on previously untreated states through a federal mandate, orthogonal to any state-level variation in awareness. If the supply side is more influential—if awareness, not the legal framework, drives the baseline results—then the federal act should have little effect on female hiring among firms in previously untreated states, where awareness was presumably lower. In contrast, if the demand side is correct, the act should reduce female hiring by raising these firms' expected legal costs, just as the state-level laws did.

We estimate a difference-in-differences model over the 2020–2024 period. The key variable is the interaction $NDAWeakenFed_i \times Post_t$, where $NDAWeakenFed_i$ equals one for startups headquartered in states that had not enacted NDA-weakening laws by 2020, and $Post_t$ equals one for years after 2022, when the federal act took effect. All regressions include startup and year fixed effects.

[Insert Table 9 Here.]

Table 9 reports the Poisson regression results. The coefficient on $NDAWeakenFed \times Post$ is -0.151 in column (1) and -0.164 in column (2), implying that the federal act reduced female hiring in previously untreated states by approximately 14–15% ($= e^{-0.151} - 1$ to

$e^{-0.164} - 1$). This magnitude is comparable to, and if anything larger than, the 7.8% baseline effect of state-level NDA-weakening laws. The estimates are stable when we exclude New Mexico and Oregon, which enacted state-level laws shortly after 2020 (column (3)), and Hawaii and Maine, which adopted state-level laws in 2022, the same year as the federal reform (column (4)). These results are difficult to reconcile with the supply-side explanation: a federal mandate that was not driven by any individual state's awareness of harassment produces the same pattern of reduced female hiring, pointing to the legal framework itself—and the resulting cost increase for firms—as the causal channel.

Two additional tests corroborate this interpretation. First, we directly control for state-level awareness of harassment by including the Google search index for the keyword "Metoo" as a regressor in the baseline model. The awareness measure has no predictive power for female hiring, and the coefficient on $NDAWeaken$ remains nearly identical to the baseline estimate (Table IA.7 in the Internet Appendix). Second, we examine whether the gender salary gap narrows after the enactment of NDA-weakening laws. A supply-side mechanism predicts narrowing: if women become harder to recruit, startups would need to offer them higher relative salaries to attract them. We find no evidence of narrowing (Table IA.8 in the Internet Appendix). One caveat of the salary analysis is that the salary information reported by Revelio is not observed compensation but rather predicted based on job characteristics and user-specific attributes, and therefore should be interpreted with caution.

5 Organizational Changes After NDA-Weakening Laws

The preceding analysis documents that NDA-weakening laws reduce female hiring, likely through a demand-side mechanism. These results capture how firms adjust on the external margin—whom they hire. But firms can also respond on the internal margin, reorganizing their existing workforce and leadership to manage the higher expected costs of harassment.

Such internal adjustments have different, and potentially offsetting, welfare implications. While reduced female hiring narrows the pipeline of women entering startups, organizational changes—promoting women to leadership and removing potential harassers from management—can improve workplace quality for women who remain. Over time, these adjustments may also feed back into the hiring margin: the cross-sectional evidence in Section 4 showed that startups with stronger internal protections and higher female representation are largely unaffected by NDA-weakening laws. If the organizational changes push firms toward stronger protections, the negative hiring effects could attenuate in the long run as startups adapt.

We document three results. First, startups promote more women to senior managerial positions after the enactment of NDA-weakening laws. Second, male managers are disproportionately likely to depart, and their subsequent job outcomes—lower pay, lower seniority, longer unemployment spells—suggest that at least some of these separations are involuntary. Third, startups do not hire more women who had been previously out of the labor force, indicating that the demand-side response documented in Section 4 extends uniformly to reentrant women. Together, these findings suggest that NDA-weakening laws prompt startups to restructure internally in ways that may, over time, rebuild the institutional capacity that insulates firms from the laws' negative hiring effects.

5.1 Promotion of Female Workers

Prior studies have documented persistent barriers that prevent women from advancing to senior leadership roles, often referred to as the glass ceiling (Azmat et al., 2025). The literature has emphasized firm-level policies and organizational culture as key levers, but whether external legal reforms can catalyze advancement remains an open question. NDA-weakening laws create an incentive for startups to elevate women to supervisory roles: doing so reduces the likelihood of male-on-female harassment in the supervisor-subordinate relationship, thereby lowering the firm's exposure to the higher legal costs documented above. This risk-mitigation channel predicts that the effect should concentrate at the manager and director level—where the glass ceiling binds and harassment risk is most salient—rather than at junior positions.

[Insert Table 10 Here.]

We estimate model (1) with the dependent variable replaced by the fraction of female workers among all workers promoted within the startup in a given year. We separately examine promotions to manager- or director-level positions (columns (3)–(4)) versus promotions to junior or associate positions (columns (1)–(2)). The sample is restricted to startup-years in which at least one promotion occurs, yielding 14,109 observations for manager-level promotions and 10,525 for below-manager promotions.

Table 10 Panel A reports the results. The coefficient on $NDAWeaken$ is 0.031 for manager-level promotions (column (4)), indicating that the female share of promotions to management rises by approximately 3 percentage points after the enactment of NDA-weakening laws. The effect is statistically significant at the 10% level. For promotions to junior positions, the coefficient is similar in magnitude but statistically insignificant (columns (1)–(2)). The concentration of the effect at the management level is consistent with the risk-mitigation channel, which operates specifically at the level of the supervisor-subordinate hierarchy where harassment exposure is greatest.

5.2 Departure of Male Managers

The promotion results raise a natural question: do the promoted women replace male managers who leave the startup, voluntarily or involuntarily? The analysis in Section 4 showed that firms reduce their hiring of potential victims—junior women—in response to higher expected harassment costs. Firms may also adjust on the other side of the harassment hierarchy by removing potential harassers from management. This could occur through two channels: firms fire managers to reduce legal exposure, or male managers self-select out as their behavior now carries greater personal risk. Both channels predict higher male manager departure rates after the enactment of NDA-weakening laws.

We estimate model (1) with the dependent variable replaced by the fraction of male managers among all departing managers in a given year. Managers are defined as employees at the manager or director level at the time of departure. Column (1) of Panel B of Table 10 reports the results: the coefficient on $NDAWeaken$ is 0.027, implying that the male share of managerial departures rises by 2.7 percentage points after the enactment of NDA-weakening laws. Importantly, we find no corresponding effect on departures of non-managerial male workers (Table IA.9 in the Internet Appendix), confirming that the effect is specific to the management level where the harassment power dynamic operates.

If these departures reflected voluntary moves to better opportunities, we would expect departing managers' subsequent job outcomes to improve. Columns (2)–(4) test this by examining the employment trajectories of departing male managers. The fraction transitioning to jobs with lower salaries rises by 3.6 percentage points (column (2)), the fraction moving to lower-seniority positions rises by 3.9 percentage points (column (3)), and the fraction experiencing unemployment spells longer than the sample median rises by 5.1 percentage points (column (4)). All three effects are larger than the baseline departure effect, suggesting that a substantial portion of these separations are involuntary—consistent with firms removing managers who pose harassment-related legal risk.

Together with the promotion results, these findings depict a pattern of internal restructuring: male managers depart—or are pushed out—and women fill the resulting vacancies in leadership. This interpretation carries a caveat. The departures are also consistent with male managers withdrawing from startups because closer professional interactions with female subordinates may expose them to accusations, a chilling effect documented in other professional settings after the #MeToo movement (Gertsberg, 2026). We cannot fully distinguish between the removal of actual or potential harassers and a broader retreat from male-female workplace relationships, though the worsening job outcomes in columns (2)–(4) suggest that at least some departures are not purely voluntary.

5.3 Effects on Re-Entrant Women

The promotion and departure results show that NDA-weakening laws catalyze internal restructuring—more women in leadership, fewer potential harassers in management. A natural question is whether these improvements translate into increased hiring of women who had previously left the labor force. If the safer workplace environment encourages discouraged women to re-enter, we would expect a smaller effect of NDA-weakening laws on the hiring of re-entrant women compared to the baseline effect on all women. We test this by re-estimating model (1) with the dependent variable replaced by the number of female hires who had been unemployed for at least two years prior to joining the startup. The coefficient on $NDAWeaken$ is approximately -0.087 (columns (1)–(2) of Table IA.10), nearly identical to the baseline coefficient of -0.081 for all female hires. The results are unchanged when we lower the unemployment threshold to one year (columns (3)–(4)). The demand-side reduction in female hiring thus applies uniformly—including to women re-entering the labor force—with no detectable supply-side offset for the group most likely to benefit from a safer workplace.

Taken together, the results in this section show that NDA-weakening laws trigger meaningful organizational restructuring within startups: more women advance to management, and male managers—particularly those who subsequently experience worse career outcomes—depart at higher rates. These internal adjustments may improve workplace quality for women who remain. Whether these organizational adjustments eventually feed back into higher female hiring—as the cross-sectional evidence on internal protections would suggest—remains an open empirical question that would be answered when longer sample periods are available.

5.4 Capital Market Responses

Why do treated startups undertake these actions? One answer operates at the firm level—rising legal exposure pushes startups to reorganize leadership to manage harassment risk directly. A second answer, distinctive to entrepreneurial firms, operates through capital markets. Venture capital investors hold board seats, exercise staged-financing rights, and bear reputational and fiduciary exposure to portfolio-firm misconduct, giving them direct incentives—and unusually concentrated tools, including the ability to withhold the next round or condition financing on governance terms—to demand organizational change. This contrasts with the diffuse disciplinary mechanisms in the public-firm settings studied in much of the governance literature (Bena et al., 2022; Adams and Ferreira, 2009).

[Insert Table 11 Here.]

We test for a capital-market response in Table 11. The dependent variable in columns (1) and (2) is an indicator equal to one if the startup receives any VC funding in the year; in columns (3)–(4) it is the number of financing rounds. The coefficient on $NDAWeaken$ is negative and significant at the 1% level in all four columns. NDA-weakening laws lower the probability of receiving any funding by 2.8 percentage points (a 9% decline relative to the sample mean of 31%) and reduce the number of financing rounds by roughly 12% ($e^{-0.129}-1$).

We caution that three factors could mitigate the VC pressure interpretation. First, treated startups hire fewer female workers (Section 4), so part of the funding decline could reflect reduced *demand* for capital. The magnitudes make a purely mechanical interpretation unlikely: an 8% reduction in female hiring, with no detectable change in male hiring, would mechanically lower total hiring by roughly 3%—well below the 12% drop in rounds. Second, the funding decline is consistent with two related capital-market mechanisms: VCs disciplining portfolio firms toward organizational change, and VCs simply limiting their own legal and reputational exposure. Both constitute capital-market pressure, and our reduced-form estimates cannot separate them. Third, this could be a result of poorer performance of startups in treated states due to limiting female hiring, which would reduce their ability to raise capital.

We read these patterns as evidence that NDA-weakening laws reshape treated startups along multiple, mutually reinforcing margins—labor, leadership, and capital—with VC capital-market pressure as one plausible channel through which the reform's pressure and costs reach inside the firm.

6 Conclusions

This paper examines the labor-market consequences of state laws that weaken non-disclosure agreements (NDAs) in cases of workplace sexual harassment. Using a staggered difference-in-differences design and linked data on over 50,000 venture-capital-backed startups from 2014–2022, we document that NDA-weakening laws reduce female hiring by approximately 8%, with effects on both the intensive margin (fewer women hired per startup) and the extensive margin (lower probability that a startup hires any woman at all). The reduction is uniform across skill levels but concentrates sharply among junior women—those most exposed to harassment—and in small, male-dominated startups that lack internal safeguards against harassment. Startups with strong existing protections and high female representation are largely unaffected.

Several features of the evidence support a causal, demand-side interpretation. Pre-trends in female hiring are flat in the four years before enactment; the effect appears sharply at the reform date and persists without attenuation. A geographic placebo test shows no decline in female hiring outside the startup's treated headquarters state, ruling out firm-level confounds. The cross-sectional heterogeneity—concentrated in firms with weak internal protections—is inconsistent with supply-side self-selection, since prospective employees generally cannot observe a startup's HR infrastructure before joining. Most directly, the 2022 federal Speak Out Act, which imposed NDA-weakening legislation on previously untreated states through a federal mandate, produces a comparable reduction in female hiring in those states. Because the federal act was not driven by any individual state's awareness of harassment, this result points to the legal framework itself—and the resulting increase in firms' expected costs—as the operative channel.

NDA-weakening laws also catalyze internal organizational adjustments. Treated startups promote more women to managerial positions, and male managers depart at higher rates. Departing male managers on average have less favorable subsequent career outcomes (lower pay, reduced seniority, longer unemployment spells) suggesting that many of these separations are involuntary rather than voluntary. These patterns are consistent with firms restructuring their leadership to reduce harassment-related legal exposure.

Like the Americans with Disabilities Act and Ban-the-Box laws, NDA-weakening reforms reduce the employment of the workers they intend to protect—but through a distinct channel. Rather than statistical discrimination under information frictions or higher firing costs, the mechanism here operates through heightened reputational and legal exposure: by allowing public disclosure of harassment, the laws raise the expected cost of employing potential victims, and firms with limited institutional capacity to manage that risk respond by hiring fewer women. The finding that well-protected startups are unaffected suggests that the policy problem is not transparency itself but the absence of complementary investments in firm-level compliance infrastructure. Indeed, the organizational changes we document—female promotions and male-manager departures—may, over time, push firms toward the stronger internal protections that insulate them from the law's negative hiring effects.

Our analysis has several limitations. The sample period ends in 2022 for the state-level analysis and 2024 for the federal test, so we cannot observe whether the initial contraction in female hiring gives way to recovery as firms adapt. The LinkedIn-based employment data from Revelio Labs, while rich in worker characteristics, may not capture the full universe of startup hiring. Whether organizational restructuring eventually feeds back into higher female hiring remains an open empirical question. More broadly, our findings underscore that transparency mandates can produce unintended consequences when imposed on institutions that lack the capacity to absorb them—a tension that extends well beyond NDAs and sexual harassment to the broader design of workplace regulation.

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Figure 1: Dynamic Effects on Female Hires

This figure plots the coefficient estimates and their 95% confidence intervals from a dynamic version of model (1), in which a treatment indicator for the enactment of state NDA-weakening laws is interacted with event-time indicators relative to the laws' effective year. The omitted (base) category is the year immediately preceding the effective year of the laws. The dependent variable is the number of female workers hired by a startup in a given year. The sample period is over 2014–2022. Estimates are obtained using a stacked regression estimator with cohort-specific startup and year fixed effects. Standard errors are clustered by startup headquarters state.

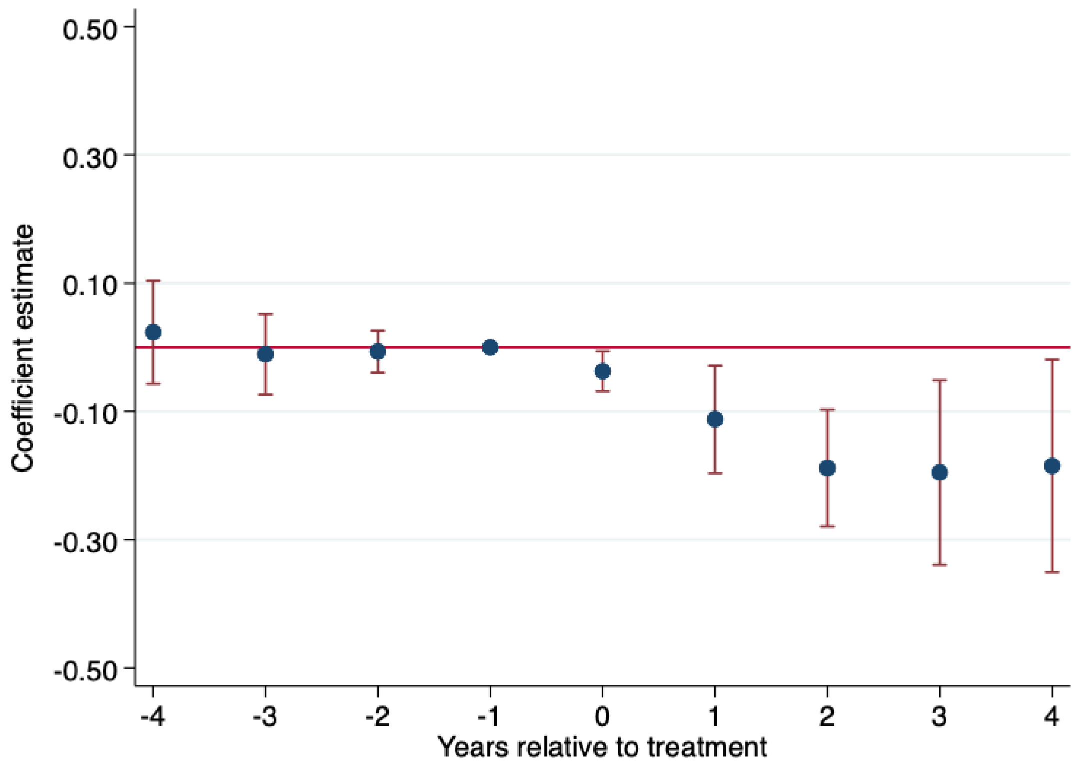

Table 1: State-Level NDA-Weakening Laws

This table lists the U.S. states that enacted laws weakening or nullifying NDAs related to sexual harassment before the enactment of the federal Speak Out Act in 2022.

State	NDA-weakening Law	Effective Date
TN	Tenn. Code Ann. § 50-1-108	2018/5/15
WA	SB 5996	2018/6/7
NY	NYS Bill	2018/7/11
VT	Act No. 183 (H. 707)	2018/7/1
AZ	Ariz. Rev. Stat. Ann. § 12-720	2018/8/3
MD	Disclosing Sexual Harassment in the Workplace Act	2018/10/1
CA	STAND (Stand Together Against Non-Disclosure) Act	2019/1/1
NJ	S-121	2019/3/18
NV	Assembly Bill No. 248 (AB 248)	2019/7/1
IL	Public Act 101-0221 (SB 75)	2020/1/1
NM	Laws 2020, ch. 16, § 1	2020/5/20
OR	Workplace Fairness Act (Senate Bill 726)	2020/10/1
HI	H.B. No. 2495	2022/7/12
ME	Nondisclosure Agreements in Employment, 26 M.R.S. § 599-C	2022/8/8

Table 2: Summary Statistics

This table presents summary statistics for the variables used in the main analyses at the startup-year level. The sample period is from 2014 to 2022. *No. Hires* is the total number of workers (both male and female) hired by a startup in a year; *No. Female Hires* is the number of female workers hired by a startup in a year; *Frac Female Hires* is the fraction of female workers hired by a startup (out of all workers hired) in a given year; *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise; *Located in Treated States* is an indicator equal to one if a startup is headquartered in a state that enacted NDA-weakening laws during the sample period; *No. Employees (Firm Size)* is the cumulative number of employees a startup has hired up to a given year; *No. VC Rounds* is the number of VC funding rounds a startup raises in a given year.

	N	Mean	Std. Dev.	5-%ile	25-%ile	50-%ile	75-%ile	95-%ile
No. Hires	207,469	2.93	9.47	0.00	0.00	1.00	3.00	12.00
No. Female Hires	207,469	1.12	4.44	0.00	0.00	0.00	1.00	5.00
Frac Female Hires	113,288	0.34	0.36	0.00	0.00	0.27	0.54	1.00
No. High-skill Female Hires (edu)	207,469	0.84	3.29	0.00	0.00	0.00	1.00	4.00
No. High-skill Female Hires (job)	207,469	0.88	3.38	0.00	0.00	0.00	1.00	4.00
No. Low-skill Female Hires (edu)	207,469	0.12	0.67	0.00	0.00	0.00	0.00	1.00
No. Low-skill Female Hires (job)	207,469	0.12	1.10	0.00	0.00	0.00	0.00	1.00
No. Junior Female Hires (age)	207,469	0.47	1.93	0.00	0.00	0.00	0.00	2.00
No. Junior Female Hires (job)	207,469	0.66	2.95	0.00	0.00	0.00	0.00	3.00
No. Senior Female Hires (age)	207,469	0.09	0.62	0.00	0.00	0.00	0.00	1.00
No. Senior Female Hires (job)	207,469	0.47	2.19	0.00	0.00	0.00	0.00	2.00
NDAWeaken	207,469	0.30	0.46	0.00	0.00	0.00	1.00	1.00
Located in Treated States	207,469	0.59	0.49	0.00	0.00	1.00	1.00	1.00
Firm Age	207,469	2.29	1.67	0.00	1.00	2.00	4.00	5.00
No. Employees (Firm Size)	207,469	12.78	49.55	0.00	1.00	4.00	11.00	46.00
No. VC Rounds	207,469	0.96	1.06	0.00	0.00	1.00	1.00	3.00

Table 3: Effects of NDA-Weakening Laws on Female Hires

This table presents regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. All columns except column (6) present Poisson regression results, where the dependent variable is the number of female workers hired by a VC-backed startup in a year, while column (6) reports an OLS specification in which the dependent variable is the fraction of female workers hired by a startup (out of all workers hired) in a given year. Column (3) restricts the sample to startup-years after a startup starts hiring (i.e., *active*). Column (4) restricts the sample to the pre-COVID period (2014–2019). Column (5) excludes startups headquartered in California and New York, two major VC hubs that have enacted laws weakening NDAs. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires					Frac
Model:	Poisson					OLS
Sample:	Full	Full	Active	Ex. COVID	Ex. CA & NY	Full
	(1)	(2)	(3)	(4)	(5)	(6)
NDAWeaken	−0.081***	−0.081***	−0.061***	−0.035**	−0.132***	−0.012***
	(0.024)	(0.023)	(0.021)	(0.017)	(0.040)	(0.004)
Log Firm Age		0.949***	0.644***	0.970***	0.924***	0.111***
		(0.032)	(0.033)	(0.041)	(0.056)	(0.005)
Observations	207,469	207,469	152,964	132,115	111,330	113,288
Adj./Pseudo R^2	0.560	0.564	0.554	0.587	0.514	0.240
Startup FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 4: Female Hires by Employee Skill Level

This table presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the number of high- or low-skilled female workers hired by a VC-backed startup in a year. *High-skilled* workers are defined as those with a bachelor's degree or higher (column (1)) or those with O*NET job codes in Job Zones 3-5 (column (2)). *Low-skilled* workers are defined as those without a bachelor's degree (column (3)) or those with O*NET job codes in Job Zones 1-2 (column (4)). Columns (5) and (6) test for differences in the estimated coefficients between high- and low-skilled female hires, using educational attainment and job zone classifications, respectively. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires					
Skill:	High Skill		Low Skill		High - Low	
Proxy:	Edu	Job	Edu	Job	Edu	Job
	(1)	(2)	(3)	(4)	(5)	(6)
NDAWeaken × High Skill					0.057	0.079
					(0.039)	(0.062)
NDAWeaken	–0.066***	–0.069***	–0.123***	–0.148**	–0.123***	–0.148**
	(0.024)	(0.022)	(0.038)	(0.066)	(0.038)	(0.066)
Observations	207,469	207,469	207,469	207,469	414,938	414,938
Pseudo R^2	0.520	0.516	0.263	0.441	0.507	0.515
Startup	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 5: Placebo Test: Female Hires in Non-Headquarters States

This table reports placebo test results for the baseline estimates using female hires at startups' non-headquarters states. It presents Poisson regression results using startup-year observations over the 2014–2022 period. The sample includes the first five years after the founding of each startup. The dependent variable in columns (1)-(2) is the number of female hires in a startup's non-headquarters states, while in columns (3)-(4) it is the number of female hires in the startup's non-headquarters states that have never enacted NDA-weakening laws. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			
Job location:	Non-HQ States		Never-treated States	
	(1)	(2)	(3)	(4)
NDAWeaken	0.023	0.026	0.018	0.023
	(0.060)	(0.065)	(0.066)	(0.071)
Log Firm Age		0.818***		0.808***
		(0.116)		(0.118)
Observations	207,469	207,469	207,469	207,469
Pseudo R^2	0.690	0.692	0.653	0.654
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 6: Extensive Margin Analysis

This table presents extensive margin analyses examining VC-backed startups' likelihood of hiring a female worker following the passage of state NDA-weakening laws, at the startup level. All regressions are estimated using OLS. The dependent variable equals one if a startup hires a female worker within the indicated time window after treatment—one year (column (1)), two years (column (2)), three years (column (3)), or four years (column (4))—and zero otherwise. For each treatment year (i.e., 2018, 2019, 2020, and 2022), the sample includes startups that (i) are headquartered either in treated states with the corresponding treatment year or in never-treated states, and (ii) had not hired a female worker prior to the treatment year. We then stack the samples across the four treatment years. *NDAWeaken* equals one if the startup is headquartered in a state that enacted NDA-weakening laws, and zero otherwise. All regressions include treatment year and firm age fixed effects. *Frac Women* is the fraction of VC-backed startups in the same state-age group that had at least one female worker prior to treatment. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Likelihood to Hire Female			
Post window:	1 yr	2 yrs	3 yrs	4 yrs
	(1)	(2)	(3)	(4)
NDAWeaken	−0.028**	−0.040***	−0.047***	−0.049***
	(0.011)	(0.012)	(0.012)	(0.013)
Frac Women	0.488***	0.548***	0.583***	0.598***
	(0.048)	(0.070)	(0.084)	(0.087)
Observations	28,644	28,644	28,644	28,644
Adj. R^2	0.043	0.065	0.085	0.097
Treatment Year FE	Y	Y	Y	Y
Firm Age FE	Y	Y	Y	Y

Table 7: Female Hires by Employee Seniority

This table presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the number of junior or senior female workers hired by a VC-backed startup in a year. Junior workers are defined as those younger than 35 years (column (1)) or those holding entry-level, or junior-level positions (column (2)). Senior workers are defined as those aged 35 and above (column (3)) or those holding associate-, manager-, director-, executive-, or senior executive-level positions (column (4)). Columns (5) and (6) test for differences in the estimated coefficients between junior and senior female hires based on the age and job position classifications, respectively. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires					
Seniority:	Junior		Senior		Junior - Senior	
Proxy:	Age	Job	Age	Job	Age	Job
	(1)	(2)	(3)	(4)	(5)	(6)
NDAWeaken × Junior					–0.067*	–0.059*
					(0.038)	(0.034)
NDAWeaken	–0.095***	–0.102***	–0.028	–0.044	–0.028	–0.044
	(0.026)	(0.027)	(0.032)	(0.032)	(0.032)	(0.032)
Observations	207,469	207,469	207,469	207,469	414,938	414,938
Pseudo R^2	0.441	0.520	0.223	0.402	0.420	0.476
Startup	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 8: Female Hires by Firm Size and Female Friendliness

This table presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the number of female workers hired by a VC-backed startup in a given year. Columns (1) and (2) restrict the sample to *Large* and *Small* startups, defined as those with total employment in the top third and bottom two-thirds of the sample distribution, respectively. Columns (4) and (5) restrict the sample to startups with *High* and *Low* female friendliness, defined based on whether the fraction of female employees (out of all employees) falls in the top third or bottom two-thirds of the sample distribution, respectively. Columns (3) and (6) estimate the coefficient differences across the two groups in each classification. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires					
Proxy:	Firm Size			Female Friendliness		
Sample:	Large	Small	Full	High	Low	Full
	(1)	(2)	(3)	(4)	(5)	(6)
NDAWeaken × Large/High			0.086***			0.109**
			(0.033)			(0.047)
NDAWeaken	–0.046**	–0.132***	–0.132***	–0.007	–0.116***	–0.116***
	(0.023)	(0.026)	(0.026)	(0.024)	(0.039)	(0.039)
Observations	69,063	138,406	207,469	55,877	116,748	172,625
Pseudo R^2	0.565	0.129	0.587	0.602	0.510	0.574
Startup FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 9: Effects of the Federal NDA-Weakening Law

This table estimates the effects of NDA-Weakening laws on female hiring using startup-year observations over the post-COVID period (2020–2024), exploiting the federal NDA-weakening reform. The dependent variable is the number of female workers hired by a VC-backed startup in a given year. *NDAWeakenFed* equals one if the startup is headquartered in a state that had *not* enacted an NDA-weakening law by 2020 (the first year of the sample period), and zero otherwise. *Post* equals one for observations after 2022, when the federal NDA-weakening law became effective. Column (3) excludes startups headquartered in New Mexico and Oregon, which enacted NDA-weakening laws in mid-2020, after the start of the sample period. Column (4) excludes startups headquartered in Hawaii and Maine, which enacted NDA-weakening laws in 2022, the same year as the federal reform. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			
Sample:	Full	Full	Ex. NM & OR	Ex. HI & ME
	(1)	(2)	(3)	(4)
NDAWeakenFed × Post	–0.151**	–0.164**	–0.167**	–0.164**
	(0.070)	(0.075)	(0.075)	(0.075)
Log Firm Age		0.847***	0.845***	0.848***
		(0.070)	(0.070)	(0.070)
Observations	113,301	113,301	112,109	112,990
Pseudo R^2	0.682	0.684	0.684	0.684
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 10: Female Promotions and Male Manager Departures

This table examines the effects of state NDA-weakening laws on the promotion of female workers and the departure of male managers. It presents OLS regression results using startup-year observations over the 2014–2022 period. In Panel A, the dependent variable is the fraction of female workers promoted in a given year, relative to all promoted workers. Promoted workers are defined as those whose job positions change to more senior roles within the same startup. Columns (1)-(2) focus on promotions to junior- or associate-level positions, while columns (3)-(4) focus on promotions to manager- or director-level positions. In Panel B, the dependent variable is the fraction of departing male managers (i.e. workers in manager or director positions) who transition to any job (column (1)), to jobs with lower salaries (column (2)), to jobs with lower seniority (column (3)), or to employment following an unemployment spell longer than the sample median (column (4)), relative to all such workers. All regressions include startup and year fixed effects (not shown to conserve space).

Panel A. Promotions of Female Workers

Dependent variable:	Frac Female Promotion			
Seniority after promotion:	Manager Below		Manager	
	(1)	(2)	(3)	(4)
NDAWeaken	0.032	0.033	0.030*	0.031*
	(0.028)	(0.028)	(0.017)	(0.017)
Log Firm Age		0.176**		0.051
		(0.068)		(0.057)
Observations	10,525	10,525	14,109	14,109
Adj. R^2	0.209	0.210	0.200	0.200

Panel B. Departures of Male Managers

Dependent variable:	Frac Male Manager Departure			
Job after departure:	Any	Lower Salary	Lower Seniority	Longer Unemp.
	(1)	(2)	(3)	(4)
NDAWeaken	0.027***	0.036**	0.039*	0.051***
	(0.009)	(0.017)	(0.022)	(0.018)
Log Firm Age	0.007	−0.042	−0.040	0.005
	(0.023)	(0.045)	(0.047)	(0.037)
Observations	47,239	24,510	23,752	22,393
Adj. R^2	0.162	0.144	0.139	0.127

Table 11: VC Funding Outcomes

This table presents analyses of the effect of NDA-weakening laws on VC-backed startup performance using startup-year observations over the 2014–2022 period. Columns (1)-(2) report OLS regression results, while columns (3)-(4) report Poisson regression results. Our sample includes the first five years after the founding of each startup. The dependent variable is an indicator equal to one if the startup raised a VC funding round in a given year, and zero otherwise, in columns (1)-(2), and the number of VC funding rounds raised by the startup in that year in columns (3)-(4). *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	New VC Round		No. VC Rounds	
Model:	OLS		Poisson	
	(1)	(2)	(3)	(4)
NDAWeaken	−0.028***	−0.028***	−0.129***	−0.137***
	(0.008)	(0.008)	(0.031)	(0.032)
Log Firm Age		0.322***		1.247***
		(0.009)		(0.041)
Observations	207,469	207,469	207,469	207,469
Adj./Pseudo R^2	0.007	0.021	0.081	0.088
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Internet Appendix

A Additional Figures and Tables

Table IA.1: Robustness: Older Startups and Female Employment Share

This table reports robustness checks for the main results in Table 3. Columns (1) and (2) extend the baseline sample to include the first seven years after the founding of the startup. Columns (3) and (4) use the fraction of female employees, rather than female hires, as the dependent variable. Columns (1) and (2) present Poisson estimates, while columns (3) and (4) report OLS estimates. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires		Frac of Female Employees	
Model:	Poisson		OLS	
Sample:	7 y.o.		Baseline	
	(1)	(2)	(3)	(4)
NDAWeaken	−0.071***	−0.063***	−0.006***	−0.007***
	(0.025)	(0.024)	(0.002)	(0.002)
Log Firm Age		0.984***		0.041***
		(0.043)		(0.003)
Observations	250,432	250,432	152,964	152,964
Adj./Pseudo R^2	0.600	0.606	0.801	0.801
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.2: Differential Effects on Female and Male Hires

This table examines the differential effects of state NDA-weakening laws on female versus male hiring at VC-backed startups. It presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. Analogous to columns (1) and (3) in Table 3, columns (1) and (2) use the baseline sample, with the number of male workers hired by a VC-backed startup in a given year as the dependent variable. Columns (3) and (4) test for differences in the estimated coefficients between female and male hiring by stacking the baseline sample. In the stacked sample, *Female* is an indicator equal to one when the dependent variable is the number of female hires and zero when it is the number of male hires. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. Regressions in columns (1) and (2) include startup and year fixed effects, while those in columns (3) and (4) include startup × gender and year × gender fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Male Hires		No. Hires	
Sample:	Full	Active	Full	Active
	(1)	(2)	(3)	(4)
NDAWeaken × Female			–0.047***	–0.059***
			(0.011)	(0.012)
NDAWeaken	–0.034	–0.001	–0.034	–0.001
	(0.025)	(0.024)	(0.025)	(0.024)
Observations	207,469	152,964	414,938	305,928
Pseudo R^2	0.566	0.558	0.566	0.558
Startup FE	Y	Y	N	N
Year FE	Y	Y	N	N
Startup × Gender FE	N	N	Y	Y
Year × Gender FE	N	N	Y	Y

Table IA.3: Stacked Regressions

This table reports a robustness check of the main results in Table 3 using a stacked regression estimator. Specifically, for each treatment year/cohort (i.e., 2018, 2019, 2020, and 2022), we construct a panel that includes startups headquartered either in treated states with the corresponding treatment year or in never-treated states. We then stack these panels across the four treatment years and estimate regressions using the combined sample. All columns except column (4) present Poisson regression results, where the dependent variable is the number of female workers hired by a VC-backed startup in a given year, while column (4) reports OLS specification in which the dependent variable is the fraction of female workers hired by a startup (out of all workers hired) in a given year. Column (3) restricts the sample to startup-years after a startup starts hiring (i.e., *active*). *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			Frac
Model:	Poisson			OLS
Sample:	Full	Full	Active	Full
	(1)	(2)	(3)	(4)
NDAWeaken	−0.092***	−0.088**	−0.058*	−0.015***
	(0.035)	(0.035)	(0.030)	(0.005)
Log Firm Age		0.915***	0.700***	0.108***
		(0.044)	(0.050)	(0.008)
Observations	459,706	459,706	407,515	237,356
Adj./Pseudo R^2	0.535	0.539	0.567	0.225
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.4: Female Hires by Firm Size and Founder Gender

This table presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the number of female workers hired by a VC-backed startup in a given year. Columns (1) and (2) restrict the sample to *Large* and *Small* startups, defined as those with total female employment in the top third and bottom two-thirds of the sample distribution, respectively. Columns (4) and (5) restrict the sample to startups with *Female* and *Male* founders, respectively. Columns (3) and (6) estimate the coefficient differences across the two corresponding groups. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires					
Moderating variable:	Firm Size			Founder Gender		
Sample:	Large	Small	Full	Female	Male	Full
	(1)	(2)	(3)	(4)	(5)	(6)
NDAWeaken × Large/Female			0.104**			–0.033
			(0.046)			(0.061)
NDAWeaken	–0.022	–0.126***	–0.126***	–0.103*	–0.070**	–0.070**
	(0.027)	(0.037)	(0.037)	(0.058)	(0.028)	(0.028)
Observations	66,086	141,383	207,469	24,466	183,003	207,469
Pseudo R^2	0.554	0.079	0.598	0.592	0.555	0.561
Startup FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table IA.5: Female Hires by Firm Size Brackets

This table presents Poisson regression results using startup-year observations over the 2014–2022 period, separately for startups in four size brackets based on total employment. Our sample includes the first five years after the founding of each startup. The dependent variable is the annual number of female workers hired by a VC-backed startup. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			
Firm size:	[1,10]	[11,20]	[21,40]	+40
	(1)	(2)	(3)	(4)
NDAWeaken	−0.116***	−0.036	−0.053	−0.075*
	(0.029)	(0.037)	(0.054)	(0.045)
Observations	119,574	25,223	15,511	12,317
Pseudo R^2	0.148	0.238	0.294	0.642
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.6: Female Hires at Public Tech Firms

This table presents Poisson regression results using firm-year observations over the 2014–2022 period. Our sample includes publicly traded tech firms, defined based on the industry classification in Goldschlag and Miranda (2020, Table 7). The dependent variable is the number of female workers hired by a firm in a given year. The first two columns use the full sample, while the last two restrict the sample to firm-year observations after a firm begins hiring (i.e., *active*). *NDAWeaken* equals one if the firm's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of firm age (in years) since IPO, and *Log Firm Size* is the natural logarithm of total assets. All regressions include firm and year fixed effects. Standard errors in parentheses are clustered by firm headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			
Sample:	Full		Active	
	(1)	(2)	(3)	(4)
Weaken NDA	0.018	0.017	0.019	0.017
	(0.070)	(0.049)	(0.071)	(0.049)
Log Firm Age		−0.078*		−0.090*
		(0.046)		(0.046)
Log Firm Size		0.387***		0.389***
		(0.046)		(0.045)
Observations	10,362	10,314	9,485	9,444
Pseudo R^2	0.911	0.917	0.910	0.916
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.7: Controlling for State-Level Social Awareness

This table provides a robustness check for Table 3 by additionally controlling for social awareness related to the #MeToo movement and sexual harassment, proxied by Google search intensity for the keyword "Metoo" (transformed using the log one plus function). The Google Trends data are obtained at the state level from 2016, the first year with nonzero search activity, through 2022. All columns except column (4) present Poisson regression results, where the dependent variable is the number of female workers hired by a VC-backed startup in a year, while column (4) reports an OLS specification in which the dependent variable is the fraction of female workers hired by a startup (out of all workers hired) in a given year. Column (3) restricts the sample to startup-years after a startup starts hiring (i.e., *active*). *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. *Startup FE* and *Year FE* denote startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			Frac
Model:	Poisson			OLS
Sample:	Full	Full	Active	Full
	(1)	(2)	(3)	(4)
NDAWeaken	−0.079***	−0.078***	−0.064***	−0.011**
	(0.020)	(0.019)	(0.017)	(0.004)
Log Firm Age		0.937***	0.612***	0.116***
		(0.049)	(0.041)	(0.006)
Log Google Searches	−0.006	−0.006	0.002	−0.002
	(0.012)	(0.012)	(0.012)	(0.002)
Observations	168,922	168,922	126,546	92,952
Adj./Pseudo R^2	0.553	0.557	0.548	0.239
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.8: The Salary Gap between Female and Male Hires

This table presents OLS regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the difference between the salaries of newly hired male and female workers, measured in thousands of U.S. dollars and defined using the median salary of all such hires by a startup in a given year in columns (1) and (2), and the mean salary in columns (3) and (4), conditional on the startup making at least one male and one female hire in a given year. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Female Salary Gap			
	Median		Mean	
	(1)	(2)	(3)	(4)
NDAWeaken	2.913	3.071	2.991*	3.166*
	(1.805)	(1.834)	(1.779)	(1.824)
Log Firm Age		–25.107***		–27.874***
		(2.746)		(2.989)
Observations	51,538	51,538	51,538	51,538
Adj. R^2	0.058	0.060	0.057	0.060
Startup	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.9: Departures of Male Workers Below the Manager Level

This table provides robustness checks for Table 10 Panel B by examining the departure of male workers in positions below the manager level. The dependent variable is the fraction of departing male workers in junior or associate positions who transition to any job (column (1)), to jobs with lower salaries (column (2)), to jobs with lower seniority (column (3)), or to employment following an unemployment spell longer than the sample median (column (4)), relative to all such workers. *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Frac Male Departure			
Job after departure:	Any	Lower Salary	Lower Seniority	Longer Unemp.
	(1)	(2)	(3)	(4)
NDAWeaken	−0.001	−0.017	−0.009	−0.006
	(0.007)	(0.012)	(0.022)	(0.006)
Log Firm Age	−0.009	−0.027	0.045	−0.015
	(0.022)	(0.034)	(0.040)	(0.025)
Observations	76,455	35,503	20,159	49,170
Adj. R^2	0.212	0.171	0.164	0.177
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA.10: Hiring of Women Rejoining the Labor Market

This table presents Poisson regression results using startup-year observations over the 2014–2022 period. Our sample includes the first five years after the founding of each startup. The dependent variable is the annual number of female workers hired by a VC-backed startup who had been unemployed for at least two years in columns (1) and (2), or at least one year in columns (3) and (4). *NDAWeaken* equals one if the startup's headquarters state has enacted a law weakening NDAs by a given year, and zero otherwise. *Log Firm Age* is the natural logarithm of startup age (in years) since founding. All regressions include startup and year fixed effects. Standard errors in parentheses are clustered by startup headquarters state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. Female Hires			
Unemployment spells:	2 yrs		1 yr	
	(1)	(2)	(3)	(4)
NDAWeaken	−0.088***	−0.087**	−0.097***	−0.096***
	(0.034)	(0.034)	(0.033)	(0.033)
Log Firm Age		0.748***		0.863***
		(0.040)		(0.032)
Observations	207,469	207,469	207,469	207,469
Pseudo R^2	0.325	0.327	0.395	0.397
Startup FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y